

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN¹*Tests afforded by the Stars*

WE will now see how the views which have been put forward are borne out by the facts which are presented to us by the stars. There is no need to occupy much time, in fact reference need only be made to Dr. Huggins' paper which was communicated to the Royal Society in the course of last year, and with that paper we may compare some earlier writings. It was as early as 1864 that Dr. Huggins, who was then associated with the late Dr. Miller, called attention to the intensely strong lines of hydrogen visible in the hottest stars.² In this paper they pointed out at the same time that other metallic lines associated with those lines of hydrogen were thin and faint. It has been already mentioned that, as we have independent evidence that these stars are hotter than our sun, we had strong grounds for believing that here we were in presence of a result brought about by a higher temperature, associated as it was with a simpler spectrum, and, therefore, presumably with simpler constituents. We need not stop now to discuss the objection which has been put forward by an ingenious person ignorant of the facts, that the broadening of these lines may not be due to an increase of temperature at all, but really to a very rapid equatorial rotation of the star. This is a fair sample of one of the classes of objections one has to meet. Of course it is at once put out of court by the fact, also stated by Dr. Huggins, that, associated with the thick lines, are excessively thin lines. Any enormous equatorial velocity of the star should have made all the lines

thick, but this is not the fact. Now we have only two lines in the solar spectrum at all comparable in thickness with these hydrogen lines in the hottest stars, taking Sirius and α Lyrae as types.

In a paper communicated to the Royal Society in 1876¹ it was remarked that laboratory work indicated the possibility that line-spectra might, after all, really not result from the vibration of similar molecules; and at that time the evidence seemed to be so clear in the case of calcium that it was pointed out that the time had arrived when evidence touching calcium itself ought, if possible, to be obtained from the stars by means, of course, of photography, because the part of the spectrum in question—the region of H and K—is exceedingly faint in the case of the stars.

Why, it may be asked, was it important to get this evidence from the stars? I will read an extract from a book,² published some years ago, which puts this view forth:—"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms, those that give us line-spectra, are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and, therefore, no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other similar atoms at all lower temperatures. Hence the association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end."

That was one point.

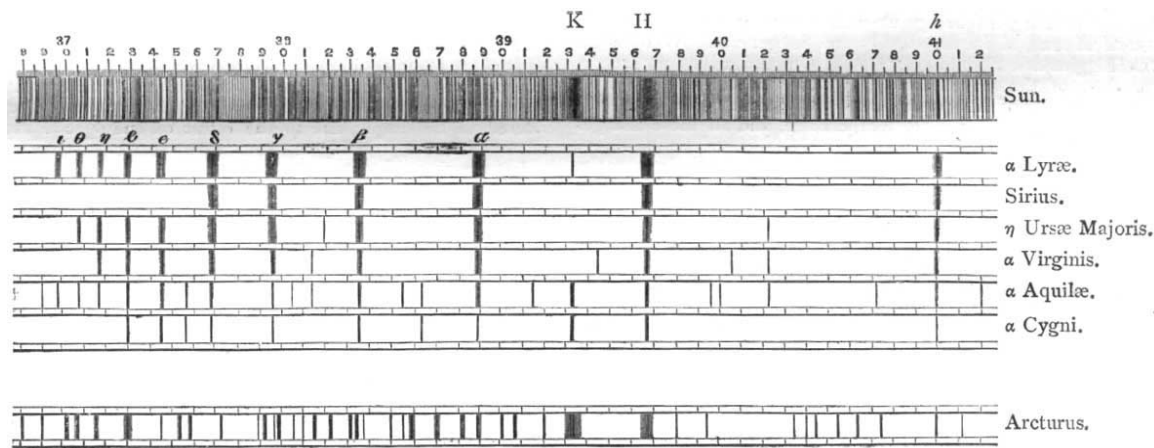


FIG. 43.—Stellar spectra (Huggins).

Here is the next point which made an appeal to the stars so necessary. "We are justified in supposing that our terrestrial calcium once formed is a distinct entity, whether it be an element or not, and, therefore, by working at terrestrial calcium alone we shall never know, even if its dissociation be granted, whether the temperature produces a simpler form, a more atomic condition of the same thing, or whether we are unable to break it up into $x + y$, because in our terrestrial calcium, assuming all calcium to be alike, neither x nor y will ever vary; but if calcium be the product of the condition of relatively low temperature, then in stars hot enough to enable its constituents to exist uncompounded we may expect these constituents to vary in quantity; there may be more x in one star, and more y in another. If this be so, then the H and K lines would vary in thickness, and the extremest limit of variation will be that we shall have only H represented, or x in one star, or only K represented or y in another, and intermediately between those extreme conditions we have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others."

What, then, are the results of this appeal to the stars which Dr. Huggins has made with such splendid success? We have in the hottest stars a spectrum so regular, so rhythmic, that it seems impossible not to consider it as produced either by the same

substance or by substances closely allied. Is it by mere accident that some of the *least* refrangible lines coincide with those of hydrogen? Is it by mere accident that the most refrangible lines have never been seen except in these stars? One of them coincides with H, one of the lines which still remain thick in the sun, and with which we find a fine line of hydrogen to be coincident. Fig. 43 is a copy of Dr. Huggins' diagram, to which reference has been made. At the top is a portion of the solar spectrum in the violet and ultra-violet, and next is the spectrum of the hottest star, α Lyrae. This spectrum, it will be seen, is simpler even than the spectrum of the solar prominences, and not only is there this wonderful simplicity, but note the exquisite rhythm by which the distance between the lines gradually increases as we go from one end of the spectrum to the other. Note also that the least refrangible line shown on the diagram is coincident with h in the violet part of the solar spectrum, and that the next line is coincident with the line H, to which reference has been made in the notes I have read. Note also the relative intensities of the lines H and K in the sun, in which their intensities are about equal, and in η Ursae Majoris, in which K is altogether absent. These are the first points in this diagram to which attention must be drawn. There will be other points as we proceed further.

But in descending from the general to the particular Dr.

¹ "Preliminary Note on the Compound Nature of the Line Spectra of Elementary Bodies" (*Proc. Roy. Soc.*, No. 163, 1876).

² "Studies in Spectrum Analysis," p. 196.

¹ Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 370.

² "On the Spectra of some of the Fixed Stars" (*Proc. Roy. Soc.*, 1864, p. 242).

Huggins writes :¹—"The spectrum of Vega may be taken conveniently as typical of the whole class of white stars, so that the distinctive features of the other stars of this class may be regarded as modifications or departures from this common typical form." He then adds : "There are principally three directions in which changes take place"; one of these consists "in the presence or absence of K, and if present, in its breadth and intensity relative to H." He goes on, "One of these modifications, which possess great suggestiveness, consists of the absence, or difference of character presented by the line K. In all the stars of this class this line is either absent or is very thin as compared with its appearance in the solar spectrum, at the same time that H remains very broad and intense. In the spectrum of Arcturus, a star which belongs to another class which includes our sun, this line K has passed beyond the condition in which it occurs in the solar spectrum, and even exceeds the solar K in breadth and intensity." Arcturus is given in the lower part of the diagram, and it will be seen that there K is relatively thicker than H; and also that with this relative increase in the thickness of K we get a considerable complexity of spectrum, very much more approaching the solar spectrum in the number of lines that we have to contend with. But at the same time I should point out that the positions of these lines vary from the positions of lines in the solar spectrum. "The spectra of these stars," Dr. Huggins continues, "may therefore be arranged in a continuous series, in which first we find this line to be absent. Then it appears as an exceedingly thin line. We then pass to another stage in which it is distinct and defined at the edges; in the solar spectrum it becomes broad and winged, and lastly in Arcturus there is further progress in the same direction, and the line, now a broad band, exceeds in intensity H." Absolute continuity we see in the story which Dr. Huggins brings us with reference to this concrete case of H and K in the details and in general. Well might Dr. Huggins ask after this : "Do these modifications not represent some of the stages through which our sun has passed?" In another part of his paper he uses the term "life changes." Now the life of a star is its temperature, and all these changes must have been produced by the running down of temperature, and I think the simplest view to take, limiting ourselves to the concrete change of H and K, is that the substance which produces K has been formed at the expense of the substance which produced H, and the reason that we see these two lines in calcium when a high temperature is employed is that we reveal the presence of these true root-forms. There may be very many more difficult explanations, but that I think is the simplest one to which one is driven by the logic of facts.

The appeal to the stars, then, I think, amply justifies the prediction which I based upon the comparison of solar with terrestrial phenomena, and, therefore, helps to show that the basis on which those predictions were founded had at all events some little glimmering of truth about it. I think also that it increases the dissociation stages through which we must assume the vapours of our so-called elements to pass when higher temperatures are employed in succession.

So much then for the tests which we have been able to apply to these views by means of Dr. Huggins' remarkably beautiful researches.

The wide departure of stars hotter presumably than the sun (taking the centre of gravity of the absorption, so to speak, as the indication of temperature) from the solar type shows that there is much more work to be done in this field. The success of my former prediction emboldens me to make another one. *It will in all probability be found that the remaining thick lines in stars of the Sirius type are represented in many cases by the lines brightened in solar prominences.*

Tests afforded by the Phenomena of Fluted Spectra

So far we have dealt with line spectra, but we must not limit ourselves to a consideration of this class of spectra if we wish to test this view to the very bottom, as it is our bounden duty to do. We have therefore to ask ourselves the question with reference to other regions of spectrum analysis beyond that particular part which we have been discussing : Is the evidence to be got from those other regions the same? Does it tend in the same direction as the evidence which has been supplied from the consideration of the highest possible temperature in our coils and in the sun? I have no hesitation in saying that, so far as I know, the evidence is absolutely strengthened by a consideration of the low temperature phenomena observed spectroscopically.

¹ *Phil. Trans.*, 1880.

In fact the view was started very many years ago by observations at much lower temperatures than those we have been considering. Plücker and Hittorf, who worked at spectrum analysis before Kirchhoff and Bunsen, were bound to acknowledge that some of the substances with which they dealt had really two distinct spectra, which they called spectra of the first order, and spectra of the second order, which spectra changed as the temperature they employed changed; and although they came to the conclusion that these simply represented allotropic conditions, not molecular dissociation, I think when one comes to inquire into the subject thoroughly, one will find there cannot be any very great difference between those two considerations. In fact the question of double spectra, which has been fought for many years, but which I think is now nearly at rest, was started by the observation of Plücker and Hittorf. Of course the view they put forward was objected to very strongly, and was met by the assertion that they were misled by impurities in the substances which they experimented on. For instance, they found a second spectrum for hydrogen; this second spectrum, which had a very special character of its own, was referred to acetylene. Soon after, a part of the carbon spectrum which was entirely different from the second spectrum of hydrogen, was referred to acetylene. So that those gentlemen who saw in these phenomena nothing but impurities were perfectly content to give an explanation which would be quite right, provided hydrogen and carbon could only be supposed to have one spectrum; the impurity acetylene having two. Later work has shown that it is too coarse a view to think that the fluted spectra which represent the spectra of the first order of Plücker really represent the vibration of one molecule in the same way that the line-spectra were supposed to do. Evidence has been accumulated which indicates that in some cases where we get three or four flutings, those three or four flutings which can be seen one by one are inversely intensified, in precisely the same way that various lines can be seen one by one, or almost one by one, and inversely intensified. It seems as if even flutings cannot be considered to be simply due to the result of one special kind of allotropism, but probably represent several.

Let me give a figure or two to represent this point.

An application of the principles referred to in Figs. 32 and 33 will readily enable us to understand that a substance may give us a particular fluting at the lowest temperature, represented by the furnace C, a different fluting in the furnace B, and finally a line at the highest temperature afforded by the furnace A, as shown in Fig. 44, and that at intermediate temperatures its spectrum may consist of mixtures in varying proportions of each of these constituents, and it will also be seen that the line produced by the highest temperature can never be seen together with the lowest temperature fluting, unless the fluting produced by the intermediate temperature is present also. Fig. 45 shows the facts actually observed when the spectrum of carbon is photographed under various conditions of temperature.

The results are strikingly suggestive, as we have a compound origin to the two sets of flutings shown.

But there is a lower region yet, a region in which much work has been done which seems to show that before the substance is fit to give us flutings, that it can still record for us—in a very feeble sort of way—it can yet record for us its vibrations by absorption at one end of the spectrum or at the other, so that the story of simplification is really intensified when we leave the high temperature spectrum, and it seems as if the first effect produced by the action of heat on any substance is to give us general absorption which breaks up into absorption in the red and absorption in the blue, and then we get a series of flutings more or less complex according to the temperature of the body; and then when we have passed from this stage we get the series of line spectra to which I have drawn attention. Again, in passing from a low temperature to a high temperature, so far as I can see there is absolutely no break, nor is there any difference of kind that we are acquainted with in the passage from a compound body and the passage from a known form of, let us say, gold or silver, at a low temperature, to that same substance at a higher temperature.

If we assume that these various spectra are really due to different molecular aggregations, we shall have the following series, going from the more simple to the more complex :—

First stages of complexity of molecule	} Line-spectra.
Second stages... ..	
	Channelled space or fluted spectra.

Third stage { Continuous absorption at the blue end not reaching to the less refrangible end. (This absorption may break up into channelled spaces.)

Fourth stage { Continuous absorption at the red end not reaching to the more refrangible end. (This absorption may break up into channelled spaces.)

Fifth stage Unique continuous absorption.

So that the story is one of absolute conformity, absolute continuity from one end of the series to the other ; but on this subject

I need not say more, because my friend Capt. Abney will have a great deal to say about the red molecules and blue molecules when he comes to deal with the red end of the spectrum, and I may safely leave this part of the subject in the hands of one who has so brilliantly distinguished himself by his investigations upon it.

Replies to Objections

Now I think it is time that I should reply, or attempt to reply, to some objections that have been made to these views. So far as I can gather, the serious objections which have been made are not many, but some of them are objections to which considerable value should be attached. The chief one now

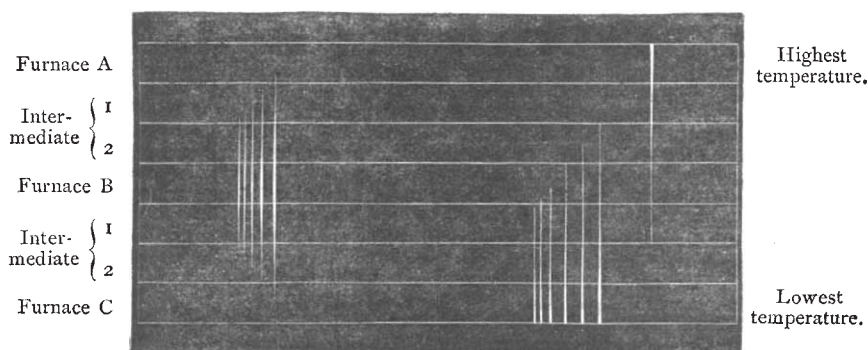


FIG. 44.—Diagram showing the action of three different temperatures on a hypothetical substance assuming three stages of complete dissociation ; and also of intermediate temperatures at which the vapours are only partially dissociated.

urged is that one is misled in the conclusions that one draws from these observations of spots and storms by the fact that the solar lines corresponding with the lines which we consider to be common to two substances are really double, and that the lines common to two substances appear common simply because we have not sufficient dispersion to separate them. Now that is a very important objection indeed, but let us examine it. It has been pointed out that of the 62 iron lines which remained as the result of the purification of the first part of the map (between wave-lengths 39 and 40) only 18 were left ; all the rest being found common, not only to two substances, but in a

great many cases to four or six substances, and we found also that our rough observation-book, as we went on, suggested that the solar line was double ; but if we had gone on in that way we should not have been able to produce a map at all, because there would have been few lines which were not complex, so that it would have been a piece of cowardice to remain there and not attempt to find out what it meant. Now let us suppose a great many of the solar lines are double. It is fair to assume that these double lines would be irregularly distributed throughout the spectrum. We cannot imagine some spiteful freak of nature choosing out to be double a particular set of solar lines which

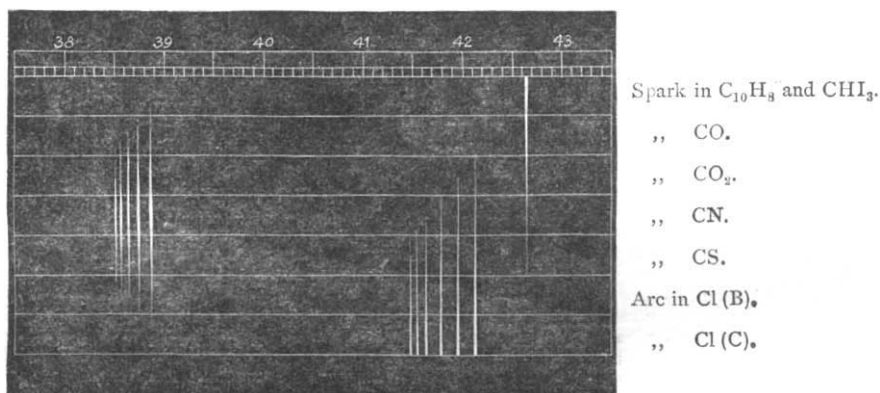


FIG. 45.—The photographed spectra of some carbon compounds.

some one should eventually find common to two substances ; we must imagine an impartial distribution of double lines if we are to consider them as double. Now the argument we can bring against that is founded on this, that two things always hang together, the alleged complex nature of the line and the fact that this particular line is picked out for special prominence in spots and storms. For instance, take the line 4923.2 referred to in Fig. 41. If that line is double, and is one of two lines seen in flames, the probability that that line should be double, supposing that the solar line is double, would be as 2 to 1, but that line is picked out as 50 to 1 in the case of prominences. The betting in favour of the coincidence is not so great in the case of the spots, but when we come to the prominences, when we are dealing with 100 lines with the probability therefore of

50 double lines if they are equally distributed, and that every other line is double—when we come from 100 lines to 1, it is 50 to 1 against this particular line being double ; and yet this is the particular line which we always find to be common to two substances when we really discuss the observations of the flames. I think then that the idea that these basic lines are simple creatures of the imagination, simply chance coincidences, will really not stand at all.

Prof. Young has lately brought forward this objection, although in 1872 he was the first to point out the very extraordinary fact as it appeared to him, and as it still appears to every one else, that an enormous number of coincident lines which he got from the tables extant at that time seemed to cluster round the bright lines seen in his observations. The credit of that is

undoubtedly due to Prof. Young, and although he has lately seen cause to withdraw somewhat from this first view of his, he is still driven to think that two of the most important lines in the solar spectrum, H and K, are really due to two substances—hydrogen and calcium. There is one more thing which must be said with special reference to this: the work is not to be negated by a mere assertion that the line is double; it must in a great many cases be shown that it is a double-double or a treble-treble line. For instance, take the case of *b₄*, which may very well be double for aught we know; that line is coincident, so far as we can make out with the means at our command, with magnesium, iron, nickel, uranium, chromium, and cerium. It will not do to limit oneself to the statement that these lines are double—they must really be perfect families of lines in order to prevent this explanation being, as I think it is, the more probable.

Another objection, again, a very important objection, is of a somewhat different nature, and may shortly be called the bell hypothesis. It is to the effect that these molecules, or atoms, are very extraordinary things indeed, as we can well imagine them to be, and that the spectrum which they produce depends entirely upon the manner in which they are struck; so that in fact it would seem at first useless to construct a map of any spectrum at all, for fear the substances we wish to observe in our laboratories should be struck in different ways and should render the map perfectly useless. I say the idea is really that the molecules struck differently would give us different spectra. Now if the difference were only slight, that would not much matter; it would be very difficult to withstand that hypothesis. But it must be remembered that in this work we are dealing with this extraordinary fact, that over the region which we have been specially studying there are no lines of iron common to spots and

flames; so that if we had not any iron at all to experiment with we should be perfectly justified in asserting the iron lines in flames were produced by one substance and those in spots by another, because no two lines agree in *these* two spectra. The spectra are as distinct as the spectrum of magnesium and the spectrum of iodine, or any other two bodies. Now if the bell hypothesis is to explain that, it explains too much, because if it is true of any two bodies, it must be true of all bodies, and therefore all spectra are the result of the same thing struck differently, and spectrum analysis would then cease to be spectrum analysis, for it would simply record changes rung on the same molecule by the various methods of striking. Then again there is another thing to be said, that no statement of this bell hypothesis which I have heard gets us out of the difficulty that we are sinning against the law of continuity in advancing it, for the reason that if you begin with a known compound body, let us say, a salt of calcium, the change from a salt of calcium to calcium is the same in kind and about the same in degree as the change from one form of calcium to the other, if we can talk of different forms of calcium on the mere strength of spectroscopic work. I mean that there are more important changes to be got out of the observations of the metal calcium than there are to be got by passing from a salt of calcium to calcium; so that if the bell hypothesis proves anything it proves that a compound body is a simple one.

It will be seen that the special import of these considerations lies in the question of the short lines; leaving all considerations dependent on long lines, by which the presence of impurities may be recognised, out of consideration altogether. But it may be further said that a method of purifying spectra and eliminating any spectroscopic defects which were due to the presence of

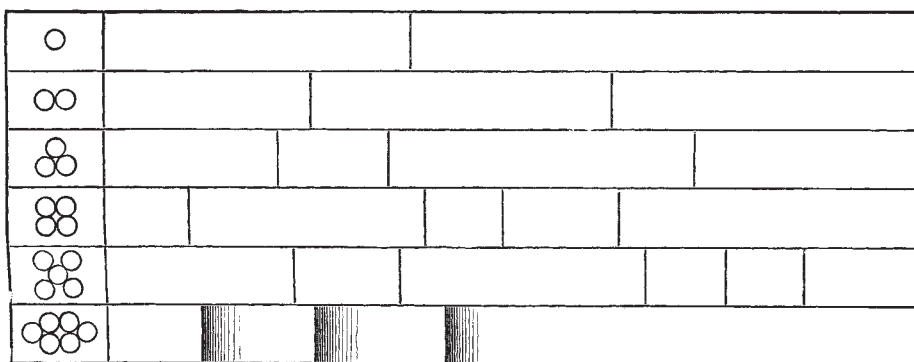


FIG. 46.—Diagram showing how the evolution of chemical forms may be indicated by their spectra.

impurities has been before the scientific world for some years, and so far as I know its validity has never been called in question. Where that method has been employed I believe it has been employed honestly, so that until that method is called in question by better work—and the work I know will be severe—I think we are bound to accept the results which have been obtained by it.

Then there are some theories which I might be permitted to say a few words about, but in reference to them I need only call attention to Dr. Schuster's admirable report on the progress of spectrum analysis, given to the British Association at the meeting at Swansea. It will be seen from that report that none of the theories which have been put forward really account for all the facts observed. It is shown that phenomena have been recorded which are not to be explained away by the bell theory or any other. Such a phenomenon, and a perfectly distinct one, is the change due to the thickness in the vapour. Changes also due to varying temperature and other causes have been seen, for which the theories in question do not account. Now changes in temperature may probably affect large reaches of the spectrum, but in the cases we have studied we got most diverse effects in lines so close together in the spectrum that it requires a considerable amount of dispersion to find out that they are really not single lines.

The New Theory of Chemical Evolution

What then is the view of the evolution of chemical species to which we are led by our study of the sun and stars? I think that after all it is but a slight expansion of the pre-

sent chemical view. Chemists regard matter as composed of atoms and molecules, about which more presently. The view now brought forward simply expands the series into a larger number of terms, and suggests that the molecular grouping of a chemical substance may be simplified almost without limit if the temperature be increased. A diagram (Fig. 46) will show exactly what I mean. If we assume a very great difference in the temperature which can be brought to bear upon a substance we may assume that at the highest temperature we have, for simplicity's sake say, a certain line represented by a single circle; let us imagine the temperature reduced, we shall then get another spectrum, which we can represent by a double circle, if we like to assume that the evolution is one which proceeds by constant additions of the original unit. Coming lower down, we get another substance formed with a more complex spectrum represented by three circles; lower down still we have one represented by four circles, another by five, another by six, and so on. We might take another supposition, easier perhaps to some minds, and suppose that evolution proceeded, not by the addition of the initial unit, but by the constant doubling of the substance of the molecule itself. Instead, therefore, of our circles increasing by one, we shall have one, two, four, eight, sixteen, thirty-two, and it will be readily understood that if there are a considerable number of stages of temperature, both within our ken and beyond our ken, and if some substances form themselves perpetually by doubling, then the unit with which we can experiment at low temperature, call it the chemical atom or the chemical molecule, or what you will, must be a very complex thing indeed. If the lower spectrum represents that of a complex

body such as iron, or a salt of calcium, the upper spectra will represent those due to the finer groupings brought about by higher temperatures. We pass continuously, as in the sun and the stars, from complexity to simplicity, if we begin at the lower stages, and from simplicity to complexity if we begin at the higher stages of temperature.

Now, two questions arise here which I think it is important to discuss. Are we playing fast and loose, in such an hypothesis as this, with the ordinary course of nature's operations, or are we in harmony with her? Again, is it contrary to the view expressed by the greatest minds which have studied chemical phenomena? I think really the view is not inharmonious with those other views which we have gathered from other regions of thought and work; in fact, I think it derives its whole force from the fact that along many lines it runs parallel with the evolutionary processes in the different kingdoms of nature. I have another diagram which will show what I mean (Table I.). This diagram deals with a very simple case of evolution, and it deals with this evolutionary process, going along a single line. Of course we know very well that in the organic kingdom evolution always proceeds along many lines, but to simplify the problem I have dealt with one of the simplest that I can think of. Let us assume that in a certain hottest star there shall be two substances, which we will call *a* and *b*. They will first at the transcendental temperature which I assume, exist as separate entities; the temperature being then reduced, they probably will combine, and, instead of two atoms, *a* and *b*, we shall have one group of *a + b*. If the temperature is still further reduced, we shall get *b* combining with *b*; in that case we shall have a grouping consisting of *a + 2b*. Let the same operation be performed again, we shall then have *a + 4b*, combining into two groups of 2; we shall have what we can represent, in short, in chemical language *a b₄*. Now, having got our *a b₂*, having got our temperature reduced, let us assume that *a b₂* is now the substance linked on to give a greater complexity, instead of *b* or *2b* merely. We then have this series given in the table.

<i>a</i>	<i>b</i>	TABLE I.
<i>a + b</i>		
<i>a + bb</i>		
<i>a + (bb)(bb)</i>		
<i>= ... a b₄</i>		
<i>+ a b₂</i>	<i>..... a₂ b₆</i>	
<i>+ a b₂</i>	<i>..... a₃ b₈</i>	
<i>+ a b₂</i>	<i>..... a₄ b₁₀</i>	
<i>+ a b₂</i>	<i>..... a₅ b₁₂</i>	
<i>+ a b₂</i>	<i>..... a₆ b₁₄</i>	
<i>+ a b₂</i>	<i>..... a₇ b₁₆</i>	
<i>+ a b₂</i>	<i>..... a₈ b₁₈</i>	
<i>+ a b₂</i>	<i>..... a₉ b₂₀</i>	
<i>+ a b₂</i>	<i>..... a₁₀ b₂₂</i>	
<i>+ a b₂</i>	<i>..... a₁₁ b₂₄</i>	
<i>+ a b₂</i>	<i>..... a₁₂ b₂₆</i>	
<i>+ a b₂</i>	<i>..... a₁₃ b₂₈</i>	
<i>+ a b₂</i>	<i>..... a₁₄ b₃₀</i>	
<i>+ a b₂</i>	<i>..... a₁₅ b₃₂</i>	
<i>+ a b₂</i>	<i>..... a₁₆ b₃₄</i>	

Now, that is an ideal scale. The question is, Is it absurd? How can we honestly answer that question? By asking whether we are or are not on the lines on which nature works in the region of the known, in the region which we can get at?

C	H	TABLE II.
C+H		
C+HH		
C+(HH)(HH)		
<i>= ... CH₄</i>		
<i>+ CH₂</i>	<i>..... C₂ H₆</i>	
<i>+ CH₂</i>	<i>..... C₃ H₈</i>	
<i>+ CH₂</i>	<i>..... C₄ H₁₀</i>	
<i>+ CH₂</i>	<i>..... C₅ H₁₂</i>	
<i>+ CH₂</i>	<i>..... C₆ H₁₄</i>	
<i>+ CH₂</i>	<i>..... C₇ H₁₆</i>	
<i>+ CH₂</i>	<i>..... C₈ H₁₈</i>	
<i>+ CH₂</i>	<i>..... C₉ H₂₀</i>	
<i>+ CH₂</i>	<i>..... C₁₀ H₂₂</i>	
<i>+ CH₂</i>	<i>..... C₁₁ H₂₄</i>	
<i>+ CH₂</i>	<i>..... C₁₂ H₂₆</i>	
<i>+ CH₂</i>	<i>..... C₁₃ H₂₈</i>	
<i>+ CH₂</i>	<i>..... C₁₄ H₃₀</i>	
<i>+ CH₂</i>	<i>..... C₁₅ H₃₂</i>	
<i>+ CH₂</i>	<i>..... C₁₆ H₃₄</i>	

We will now refer to another diagram; we will pass from the ideal to the concrete, and it will be seen that there is, if one can invert the term in such a way, a distinct precedent for such a table as the last; for here are the absolute facts with regard to one evolutionary series of the combination of carbon and hydrogen. We have the gases CH₄, C₂H₆, C₃H₈; we have as liquids from C₄H₁₀ to C₁₅H₃₂; each of them formed, not by the addition of my hypothetical *ab₂*, but by a concrete CH₂, and we have connected with that a beautiful order and exquisite regularity in the way in which the boiling-points and specific gravities of these things increase.

TABLE III.—Hydrocarbon Series

		Boiling point.	Specific gravity.
Gaseous	C H ₄	Marsh Gas.	
	C ₂ H ₆	Ethane.	
	C ₃ H ₈	Propane.	
	C ₄ H ₁₀	Tetane or Diethyl ...	1 ... '600 at 0
Liquid	C ₅ H ₁₂	Pentane ...	38 ... '628 at 17
	C ₆ H ₁₄	Hexane or Dipropyl..	71 ... '669 at 15
	C ₇ H ₁₆	Heptane ...	99 ... '699 at 16
	C ₈ H ₁₈	Octane ...	124 ... '726 at 15
	C ₉ H ₂₀	Nonane ...	148 ... '728 at 13.5
	C ₁₀ H ₂₂	Decane ...	166 ... '739 at 13.5
	C ₁₁ H ₂₄	Endecane ...	180 ... '765 at 16
	C ₁₂ H ₂₆	Dodecane or Dihexyl.	202 ... '774 at 17
	C ₁₃ H ₂₈	Tridecane ...	218 ... '792 at 20
	C ₁₄ H ₃₀	Tetradecane ...	230 ... —
	C ₁₅ H ₃₂	Pentadecane ...	258 ... '825 at 16
Solid ...	C ₁₆ H ₃₄	Hexadecane or Di-octyl ...	278 ... Melts at 21°

There is no break in the general line of increase, and after we have gone through the gaseous stage, which stops at C₃H₈, and through the liquid stage, which stops at C₁₅H₃₂, we get the solid state, and there again the same series is represented. So that I think one is justified in saying that, dealing with this one simple case (and the only reason I have taken the simple case is that it is a line which has been thoroughly worked out by organic chemists), taking this simple case we are justified in saying that if nature, in the regions which we cannot get at, works in the same way as she does in the regions which we can get at, the view is not absurd, and in fact any one who wishes to dispute the view in such a case as this has, I think, the *onus probandi* thrown upon him. He must show that either in a certain latitude or longitude, or at a certain temperature, or under some unknown condition the laws of nature are absolutely changed, and give place to new ones. That has not yet been found in any other region of natural philosophy. Indeed I think one might go further and say that all these evolutionary processes, obtained from different regions of thought, have such a oneness about them that to my mind one of the best mental images we can get of the causes which produce the lines picked out for special prominence in solar spots and solar flames, is to consider the molecular groupings that produce them as resembling the roots of the present European languages which our ancestors brought from the cradle of the race in Asia.

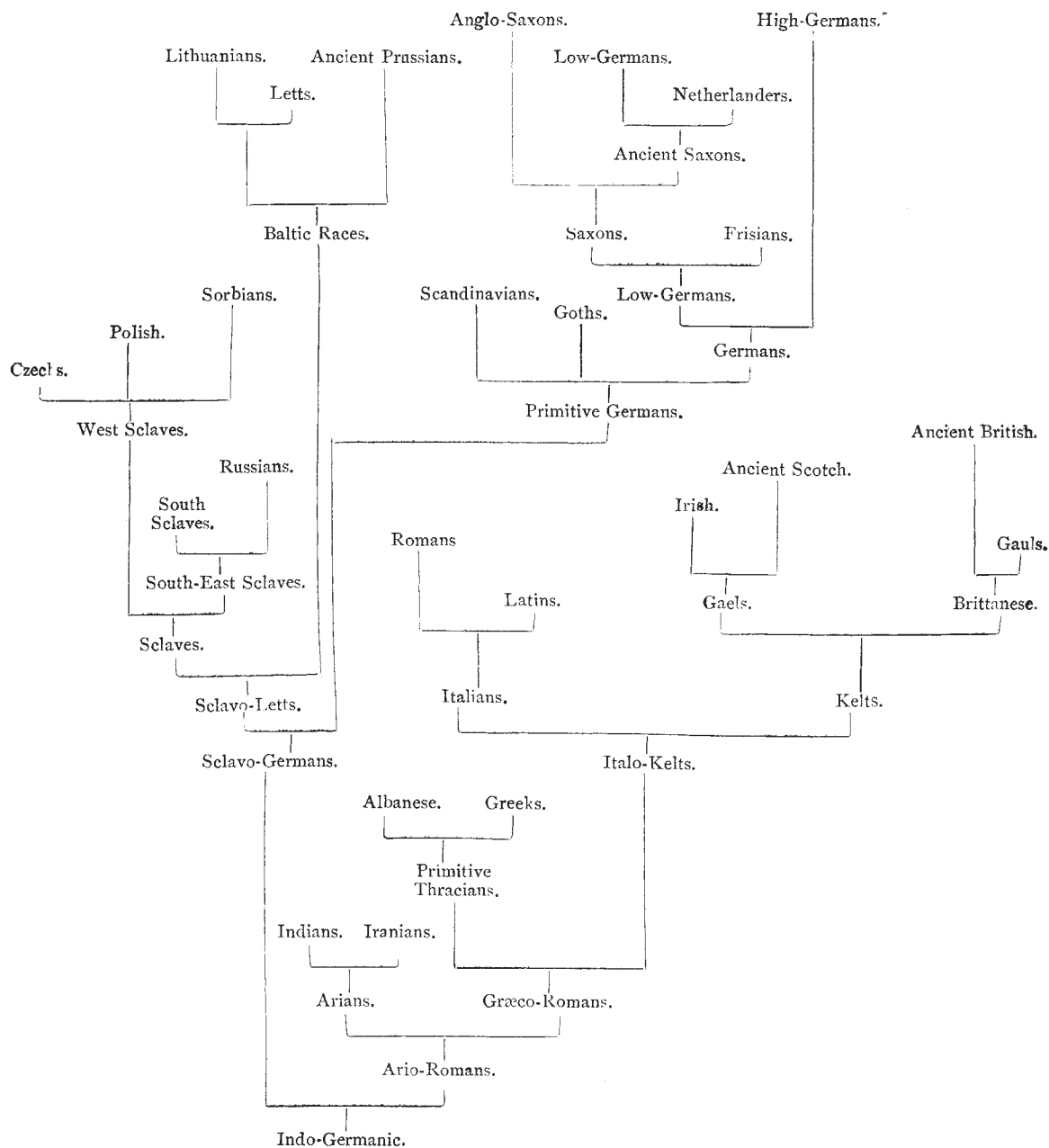
The accompanying diagram (p. 396) is taken from Haeckel's book. We begin with the European languages, including our own, High-German, and so on, and philologists have traced them down into Indo-German. We have at present in our language few and far between, the root-words which have existed almost unchanged from the time when this language only was spoken. There has been an evolution in language as there has been an evolution brought about by the reduction of temperature wherever chemical substances have been allowed to intermingle with each other, but side by side with the new chemical forms produced by the low temperature, and represented by the later languages, remain true root-forms, which may be traced to the hottest regions within our ken in the same way as the root-forms in our own language can be traced through these other forms of language back to the first one with which we are acquainted.

Now comes the second question, to which reference has been made. What is the opinion of those who have given the greatest attention to chemical philosophy? I do not mean to chemistry, I mean to chemical philosophy. We begin with Dalton. He says, "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." I go on to Graham: "It is conceivable that the various kinds of matter now recognised in different elementary substances may possess one

and the same element or atomic molecule existing in different conditions of movability. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies." I now come to Sir Benjamin Brodie, whom we have so lately lost, the last of the great English triumvirate to whom I mean to refer. His views I have already stated in his own words, but I may here again state that as early as 1867,

almost, we may say, before the spectroscope had been applied to the sun, except in the general way, which was started by Fraunhofer and Kirchhoff, he prophesied that the solar facts would be as I think we have found them. That is to say, he stated his belief that at the solar temperature the constituents of our elementary bodies would be found existing in independent forms. The greatest chemical philosopher now living, M. Dumas, so

PEDIGREE OF THE INDO-GERMANIC LANGUAGES.



long ago as 1836 published a series of lectures in which his views were very clearly stated indeed, and any one who reads them will see how convinced he was then of the considerable amount of evidence that had already been accumulated in favour of the non-elementary nature of a great number of substances then classed as elements.

Then again we can pass to another chemical philosopher, Kopp. In his researches on specific heats he also gives evidence

to show that that relationship is not to be depended upon to establish the received view. If, then, the three greatest English chemists we can name, and the most eminent chemical philosophers in France and Germany, give their opinion in behalf of the compound nature of the chemical elements, can these simpler forms be any other than those we detect by means of the spectroscope? By the conditions of the problem and the absence of knowledge they are not decomposable in the laboratory; if they

were they would cease to be elementary bodies at once, and would be wiped out of our tables. Nor do I think it possible that in the present stage of our knowledge they can be revealed to us in any other way than by the spectroscope. It is unfortunate that none of these chemists who have given us this view have helped us by showing in what way the possibility, which all of them suggest, and which many of them intensely believe in, could be absolutely demonstrated, but it is obvious that if dissociation is the thing which time out of mind has made compound bodies simpler, in their minds the condition of higher temperature must have been present. The only difficulty was the way in which the effects of that high temperature could be measured and weighed, and I think that if the spectroscope had been introduced earlier they would probably have left some hints behind them which would have been of the greatest value to those who work with that instrument.

Passing from the chemists to the physicists, there is one, at all events, who has appreciated exactly how this decomposability of the terrestrial elements could be established. I refer to the lamented Clerk Maxwell. In his article on atoms in the "Encyclopædia Britannica," he says: "The discovery of a particular line in a celestial spectrum which does not coincide with any line in a terrestrial spectrum indicates either that a substance exists in the heavenly body not yet detected by chemistry on earth, or" (and it is to the "or" I wish to draw attention) "that the temperature of the heavenly body is such that some substance undecomposable by our methods is there split up into components unknown to us in their separate states." Absolutely nothing could be clearer than this.

In endeavouring to discuss the bearing of this application of the hypothesis of evolution of chemical forms upon modern chemistry, we must draw a very wide distinction between chemical theory and chemical fact.

When we compare the laws given in average chemical text-books with the laws which lie at the root, let us say, of astronomy, the candid mind cannot fail to be struck by the difficulty which chemists must have encountered in endeavouring to reduce the facts of their science to order on the hypothesis they bring before us. An outsider, for instance, thinks that the basis of chemistry, or a large part of the basis of chemistry at all events, lies in the fact that the chemist has determined the existence of a certain number of elementary bodies, each of these elementary bodies having a certain atomic weight, and that this atomic weight determines all the constants of that body. Yet we read in chemical text-books that this atomic weight is fixed according to no invariable rule; indeed, with Kepler's laws and Newton's laws in one's mind one comes to the conclusion that it is not too much to say that it is determined by a series of compromises. An outsider would think that if any one of these elementary bodies were taken as a standard, the weight of an equal volume of vapour of another substance under equal conditions would bear some relationship of a definite character to the atomic weight. This however is not the case. Again, among the questions to be considered as determining the atomic weights taken, is an assumed limitation of combination power, a so-called atomicity, according to which one substance is a monad, because it will combine with that same relative proportion of hydrogen which exists in half a water-molecule. Another substance is called a dyad, because it will combine with the same relative proportion of hydrogen which exists in a whole water-molecule, and so on. When we thus begin to class the substances into monads, dyads, hexads, and so forth; in fact, when we thus effect a re-classification of elementary bodies, the solidarity at once breaks down; we find that the classification after all is useless, because the same substance may behave as a dyad, a tetrad, a hexad, a pseudo-triad, a pseudo-octad; in fact, one feels one is dealing with something that is more like a moral than a physical attribute—a sort of expression of free will on the part of the molecules. We are, I think, justified in asking whether these various attempts to formulate a science do not break down after a certain point, because they attempt to give a fixity to what is in truth variable.

When we pass to the facts of the science, the key-note of which is variability from one end of the scale to the other, we find that the view of successive dissociations, the view of variable molecular groupings brought about under different conditions, is really more or less in accordance with the facts where the laws based on the fixity of the facts break down entirely. Thus, for instance, let us take the question of vapour densities. The view

accounts fully for the so-called anomalous vapour densities, and in this way: it suggests that the elements may really be complex groups which break up into their constituent groups under suitable conditions of temperature, like phosphoric chloride and many other bodies do when obtained in the condition of vapour. We have dissimilar groups in the one case, and possibly similar groups in the other. In this way, that contradiction in terms, the "monatomic molecule," really becomes the evidence of a higher law.

Let us pass to allotropic conditions. The explanation of these is that there are bodies which have a large molecular range within the ordinary temperatures at our command. The substances in which allotropism is most marked are all metalloids which have not been found in the sun, and the allotropic forms give us in many cases different spectra—spectra indicating a considerable complexity of the molecules which produce them—spectra of continuous absorption, continuous radiation in the blue, continuous absorption in the red, fluted spectra, and the like. In the passage from one allotropic condition to the other, energy, without any known exception, is absorbed or given out. What becomes of this energy; what is it doing; unless it is in some way or other controlling the passage from one molecular group to another? These allotropic conditions, occurring very obviously to us in certain limits at our ordinary temperature and pressure are, I hold, but special cases of group-condensation common to all bodies, represented by Dalton's law of multiple proportions. We can indeed imagine a condition of things in which the difference between iron in Fe_8 , and the iron in Fe_2Cl_6 , would be as obvious as the difference between ordinary and amorphous phosphorus.

In certain classes of so-called organic substances this grouping of simpler groups to more complex actually takes place, and is recognised under the term polymerism—for instance, with cyanogen compounds of oxygen we have a simple thing like CNO say, which will form a series of compounds, and we have its so-called polymers, $\text{C}_2\text{N}_2\text{O}_2$, or $\text{C}_3\text{N}_3\text{O}_3$, which will each form a series of compounds, these groups of simpler nature forming by their combination group individuals with related but not identical properties with the simplest or fundamental group.

In many cases the amount of this condensation may be determined by the vapour densities. In others, again, a dissociation takes place at a certain limit of temperature, a simpler or fundamental group being the resolution product.

The resemblance between these cases of polymerism and especially those elementary bodies which exhibit allotropism, is at least striking.

In the one case, the organic complex bodies, the range of existence is in most cases within our easy attainment; in the so-called elementary stuffs it is less frequently the case. We can certainly convert ordinary phosphorus and sulphur into allotropic and most likely polymeric forms, but we do not know as yet how many atoms more are contained in the polymeric forms of these substances than in their simpler states.

And in other substances this range of condition of formation passes gradually out of our reach, but the phenomena are the same in kind up to the temperature of the sun. And again, when we can obtain the spectra of bodies like amorphous phosphorus we can prophesy that the relative grouping of the atoms of phosphorus in this to the ordinary form will be exhibited.

This brings us to the next point—atomicity. What are the associated phenomena? Lowest melting-point, simplest spectrum, lowest atomicity. Therefore we are justified, I think, in assuming that atomicity may after all be but the measure of the molecular groupings at work. In this way we can associate various atomicities, not with moral phenomena as regards the "behaviour" of the same molecule, but with different physical states—different complexities of the same substance. Thus in the same substance the more complex or allotropic the molecular grouping, the higher the atomicity. Hence the substances in which the highest atomicities appear should, as a rule, be formed and broken up at the lowest temperature. This, I am informed, is really what happens in the majority of cases.

New Analogies between Organic and Inorganic Bodies

I have ventured in these few remarks to touch upon the relations of the new view to modern chemical facts, because I think such a discussion shows us that there are several chemical regions in which the views can be tested from a chemical point of view, although I have, from a set purpose in my lectures, dealt with them absolutely from the physical side. In fact, one such

step of the highest interest has already been taken by my colleague, Capt. Abney. I will read what it is, and the language of Prof. Roscoe, the President of the Chemical Society, is so clear and so admirably put, that it is impossible for any one to improve upon it.¹ Referring to the work which Capt. Abney and Col. Festing have done together, he says: This work "is no less than a distinct physical test of the existence in organic compounds, of the organic radicals, and a means of recognising the chemical structure of an organic compound by means of the spectroscope." This result "is accomplished by photographing the absorption spectra of organic compounds in the infra-red part of the spectrum. In these invisible portions characteristic and distinct absorption lines and bands occur for each organic radical. The ethyl compounds all show one special ethyl band; the methyl compounds a special methyl band; and thus, just as a glance at the luminous portion of the spectrum satisfies us of the presence of calcium, lithium, and rhubidium, so a simple inspection of these infra-red photographs enables us to ascertain the presence of the various organic radicals. This invention is still in its infancy, but one of greater importance to chemists has seldom if ever been communicated to the Society." I have been the more anxious to give these results in Prof. Roscoe's own words, because it will be seen that, *mutatis mutandis*, these remarks touching the spectra of organic radicals are precisely the statement I have been endeavouring to make with regard to inorganic radicals. It cannot therefore be said that the nature of the principle I bring forward is one with which chemists are not

familiar. In this beautiful work, then, we have an analogy between the behaviour of known compounds and assumed elementary bodies.

A new method of laboratory work which I have recently started may, I think, in the course of time furnish us with another analogy, and in connection with it there is an experi-

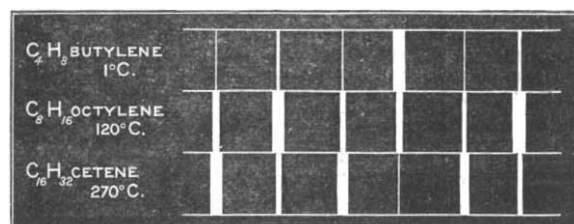


FIG. 47.—Hypothetical spectra obtained on distilling at successively increasing temperatures a mixture of light and heavy hydrocarbons.

ment to which reference may advantageously be made, because it will show what kind of results we expect to get. It is simply referred to as an indication of the probable fruit which will come from many new kinds of experimentation which might be adopted, provided always we bear in view the idea which it has been my duty to bring forward. This experiment is founded on

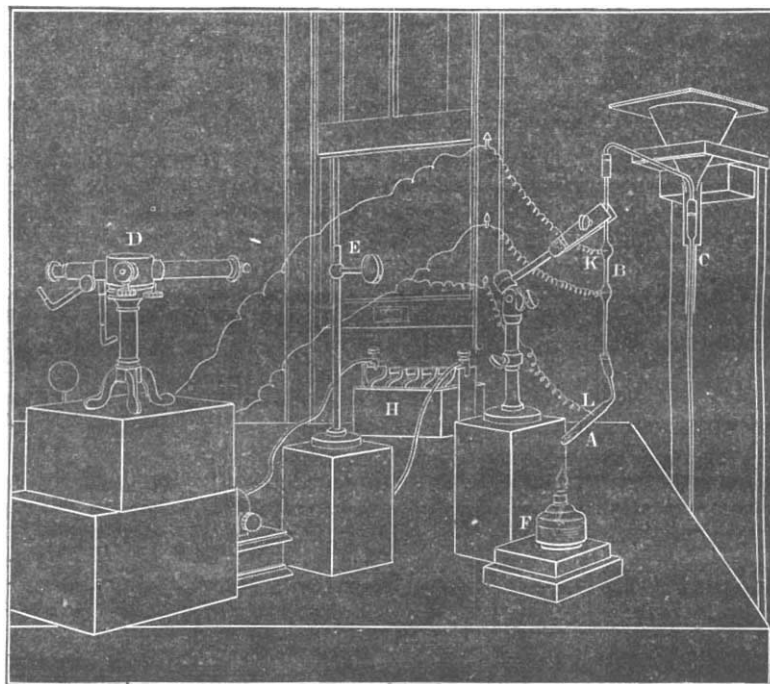


FIG. 48.—Fractional distillation of potassium. A, hard glass tube containing the potassium and connected with a Sprengel pump C by a tube B, having two bulbs with platinum electrodes sealed into them, between which an induced current may be made to pass; F, spirit lamp; H, battery; D, spectroscopic; E, lens to focus image of spark on slit of spectrocope.

the behaviour of compound bodies when they are distilled at different temperatures. If we take, for instance, a mixture of hydrocarbons, some of them very complex in their nature, and others more simple; when a low temperature is employed it is found that the simpler hydrocarbon comes over in the shape of vapour. If therefore we were fortunate enough to be able to observe the spectra of these different vapours, assume that that series of hydrocarbons, for instance, shown in the accompanying diagram (Fig. 47), had each of them a distinct spectrum, we should be able to follow spectroscopically the effect of each change of temperature, and we could in that way associate the known fact of the greatest density of the vapour which comes over at a higher temperature with a spectrum of a certain kind.

¹ *Journal of the Chemical Society*, May, 1881.

Now in our experiment we deal not with a compound body in the ordinary sense, but with the so-called elementary body, potassium, which we have in a hard glass tube of the form shown in Fig. 48. By means of a Sprengel pump the tube is very perfectly exhausted, and then gently warmed with a spirit lamp, the exhaustion going on during the whole process. On passing a current between the platinum electrodes we see a beautiful green glow in the tube, and obtain a certain spectrum. On replacing the spirit-lamp by a Bunsen burner we find as the result of this increased temperature that the colour in the tube is blood-red, and the resulting spectrum is entirely different. The spectrum of potassium is one which requires a very great deal of study, for the reason that it varies very much under different experimental conditions. If the

potassium, for instance, is thrown into a Bunsen burner, the chief line that one gets is a red one. Kirchhoff, in the early days of solar chemical investigation, pointed out that this red line is not to be found among the Fraunhofer lines. The flame also gives us a line in the blue. If we examine the spectrum of potassium by means of an induction-coil we find the blue line which we also see in the flame, but it is intensified in the spark. We also see some strong lines in the green and yellow, which are barely visible in the flame—which are in fact not generally recorded in the flame-spectrum of potassium, although they are really visible when considerable dispersion is employed. These lines in the yellow and green I say become prominent lines. Now, it so happens that some of these lines in the green do, it is believed, correspond with Fraunhofer lines, and we are, therefore, justified in assuming that they represent a something, whatever it may be, in the potassium, which can withstand the heat of the sun, while the red lines represent something which

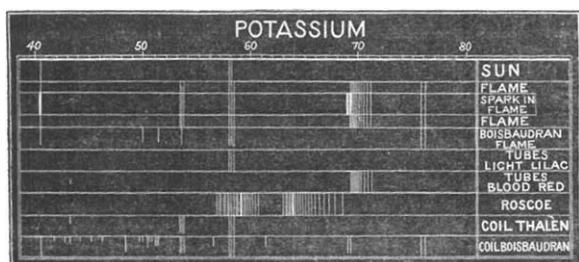


FIG. 49.—Spectra of potassium obtained under different conditions.

is broken up at the temperature of those regions of which we can determine the absorption. The interesting point of the experiment, therefore, is this: assuming for a moment that the red line does represent a complex something which cannot withstand the temperature of the sun, and that the yellow line represents a something finer which can withstand the temperature of the sun, what happens when we try to drive off the vapour of this potassium at the lowest temperature at which we can get it to volatilise at all, is that if the experiment is carefully performed it gives precisely those lines which are reversed in the solar spectrum alone, and of that line, which is the strongest line at the temperature of the Bunsen burner we see absolutely nothing at all. Referring to the spectrum which we get in the lilac and yellow-green part of the tube, two out of the three lines visible at all events are seen in the sun, whereas the other lines which we get in the flame and some of them which we get in the coil are not represented in that fine vapour which was produced at the lowest possible temperature.

The Bunsen burner produces some very exquisite colour-effects in the tube, and especially develops a beautiful blood-red colour which might be imagined to be the product of that molecule which gives the red line in the Bunsen burner; but that is not the fact. The line seen in the Bunsen burner is not visible as a rule in the vapour when heated in this way, the lines actually seen being more refrangible. Fig. 49 is a map of the spectrum of potassium under these various conditions. I give it simply as an indication that it is possible when other laboratory and chemical experiments are made with this view in mind that other analogies than those already obtained will be forthcoming.

The experiment then comes to this. If we assume the potassium to be a compound body and that its finer constituent molecules are those which resist the solar temperature, then it behaves exactly like a mixture of hydrocarbons is known to do, that is, the finer vapours come off in greatest quantity at the lowest temperature, and the more complex ones as the temperature is raised.

Conclusion

In concluding my lectures in this course on Solar Physics I would ask attention to the fact that the views which I have ventured to put forward, as being what I honestly believe to be the true outcome of the twenty years' work which has been applied to this subject, depend for their strength upon the convergence of very various lines of thought and work. No doubt the future progress of science will show that we, after all, are looking through a glass darkly, and that we are not yet face to face with the truth, and the whole truth. We must all of us be content to have our work criticised and expanded by

future work, by researches carried on with greater skill, with more elaborate methods and higher views. But with all these reservations I do wish to draw attention to the fact that the convergence of many lines of work and many lines of thought suggest the ideas which I have put forward. Depend upon it, that we shall get a much higher and much richer truth out of further inquiries; and I quite acknowledge, although I have had a hand in the work myself, that the outcome of the work is so important that it ought to be considered honestly and carefully from every point of view. Still I consider that I am in honour bound to say, as the result of the work on solar physics, in that small branch of the inquiry into solar matters with which I am more personally connected, that my belief is that the late work has changed the views which were held say twenty years ago to this extent: whereas twenty years ago we imagined ourselves to be in full presence in the sun of chemical forms with which we are familiar here, I think in this present year we are bound to consider that that view may be modified to a certain extent, and that we are justified in holding the view that, not these chemical forms, with which we are acquainted here, but their germs really, are revealed to us in the hottest regions of the sun.

J. NORMAN LOCKYER

NOTES FROM THE MALAY ARCHIPELAGO

A CORRESPONDENT in Java sends us the following:—

In 1879 I saw, at Tabu Breio, Padang Panjang, west coast of Sumatra, a child aged about one and a half years, with four legs. It was a female child with perfect organs, only the feet were clubbed and the legs bent. The added-on pair of legs were less perfect and their circulation evidently not in order, for they were not so sensitive to pain (pinches, &c.). They looked as if part of an embryo male child. The child was subject to fits; it could not walk, but crawled, using its female legs, the male (?) legs being dragged along. The spine was much dragged out of position.

During about six months of 1880 there was a child at Surabaya, Java, with two distinct heads joined to one neck. It is now with the Regent (a Javanese) of Surabaya, in spirits. Photographs of this are sold. The brains were quite independent of each other, for the one would sleep whilst the other was awake. I have not heard whether the one could articulate whilst the other slept.

Bornean Rhinoceros.—Mr. Bartlett writes to me: "We now know for certain that the Bornean is the same as the Sumatran. This comes of course from Hart Everett, and I do not doubt it for a moment." But I have strong grounds for believing that there are two kinds:—1. A Government official who recently spent a year in the deepest recesses of the island says the natives told him there were two kinds. 2. About eight years ago a small rhinoceros was killed at Bunut, about 150 miles above Sintang, on the Pontianak. This is certain, it had only one horn. I have recently spoken to an officer who spent a year and a half in the interior, and he says he always understood the animal had only one horn. Anyhow it is very rare indeed. No European I have met—and many have been a long way into Borneo—has seen it. That may be because they are phlegmatic Dutch, and not inquiring English. But the natives who killed the one at Bunut had never seen one before. At the first sight they fled in terror at such a beast. It might have been a young *R. Sum.*, as the horn was very small, and perhaps the trifling development of bud horn escaped notice.

A Dutch ship, the *Batavia*, has at length reached the point where the 141st degree cuts the west coast of New Guinea. This is considered a great feat; why, I can't precisely say. There has been a good deal of talk about sending explorers to the Dutch end of New Guinea, but directly money is asked for silence reigns. They had much better finish with Sumatra before going to New Guinea.

The cattle plague has been raging in the west end of Java, Bantam, the Preanger, and Batavia residences—during the west monsoon (now finishing) with redoubled vigour. It has now abated a little (after four years it may well do so, from want of victims) in these parts, but is extending eastward, its appearance in Krawang being the most alarming. The authorities have decided upon making a double fence right across Java at its narrowest part. This means a line from somewhere about Cheribon due south. In the interval—a considerable one—